

On Famine and Measuring 'Famine Deaths'

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There has been a long discourse on famine in the history of India. The students of her colonial history between 1765 and 1947, starting from the nationalist writers, have always made a distinction between the famines of the earlier type arising from local crop failures and inadequate systems of movement of supplies from surplus areas, on the one hand, and on the other, the secular tendency to famines arising from the specific operation of colonial surplus extraction, in particular through the revenue system. This system, it is argued, imposed not only a higher burden of land revenue than did earlier indigenous fiscal practices, but this higher demand was required to be paid at rigid intervals in cash regardless of the actual fluctuating state of the harvest. The resulting debt-enforced changes in the cropping pattern away from foodgrains towards export crops, and the enforced marketisation of surplus even for foodgrains, left no or little buffer for tiding over dearth. This produced an endemic vulnerability of peasant producers to famine.¹

The frequency and severity of famine in the late nineteenth century was a strong confirmation of this analysis. In the first half of the twentieth century, for which period systematic all-India data are available for British India, it was found that per capita food production declined by nearly 30 per cent in the inter-War period, while per capita export crops output registered rise. This pattern for India, we find, is replicated for every colonial situation: we find an inverse relation between exports and food production for the local population whether we look at the experience of Java under the Netherlands, Ireland under England, or Korea and Formosa under Japan, to cite only a few examples (Patnaik, 2000). High rates of growth of exportables were associated with low or negative growth of food crops. In India the severest fall took place in Bengal with a 38 per cent estimated decline in per head food availability between 1911 and 1947 (see Blyn, 1966; Sivasubramanian, 1965). It has been argued elsewhere by this author that this large secular fall in per head food consumption in rural Bengal implying a weakening of the disease-resisting capacity of the mass of the rural population, has to be taken into account if we are to explain the high mortality in the subsequent famine of 1943, even though the proximate cause of the famine no doubt was the impact of the highly inflationary War financing burden imposed on India (Patnaik, 1991).

The expectation of life at birth in India was estimated to have been a pathetic 24 years at the turn of the century in 1901, which further declined to a mere 20.9 years by 1921. The rapid war-time inflation eroding real incomes and the great influenza epidemic played their part in this abysmal record. The infant mortality rate (IMR) too rose during the war years to 219 from an already very high 204 in the period 1911–15 (see Table 2.1). In the next two decades the IMR declined to 161 and the expectation of life improved to reach nearly 32 years by 1941 and both indicators remained at around that level thereafter until Independence. During the first decade after Independence, 1951 to 1961, the crude death rate averaged 22.8 per thousand; its decline was very slow and it averaged 19.0 during the next decade, 1961–71, whereas China starting from a higher initial level had already brought the death rate down to 12 or so by the end of the 1960s despite the rise during the famine of 1959–61, which we discuss in more detail later. Similarly, while India reduced its IMR from 160 before Independence to 85 by 1989–90, China starting from a higher initial level had already reached this point two decades earlier, by 1970. With regard to life expectancy, there was

Table 2.1
Infant Mortality Rate and Life Expectancy (1911–93)

Period	Rural	Urban	Combined	Life Expectancy in Years		
				Census Year	M	F
1911–15	—	—	204	1901	23.6	24.0
1916–20	—	—	219	1911	22.6	23.3
1921–25	—	—	174	1921	19.4	20.9
1926–30	—	—	178	1931	26.9	26.6
1931–35	—	—	174	1941	32.1	31.4
1936–40	—	—	161	1951	32.5	31.7
1941–45	—	—	161	1961	41.9	40.6
1946–50	—	—	134	1971	46.4	44.7
1951–61	—	—	146	1980	54.1	54.7
1971	138	82	129	*1981–86	55.6	56.4
1976	139	80	129	*1986–91	58.1	59.1
1981	119	62	110	*1991–96	60.6	61.7
1983	114	66	105	*1996–2001	62.8	64.2
1986	105	62	96			
1987	104	61	95			
1989	98	58	91			
1990	86	50	80			
1991	87	53	80			
1992	85	53	79			
1993	82	45	74			

Source: *Health Information of India 1994*, Govt. of India, Ministry of Health and Family Welfare. IMR estimates for 1911–15 to 1941–45 are from Kingsley Davis, *Population of India and Pakistan*; for 1946–50, from S. Chandrasekhar, *Mortality in India*; for 1951–61 from the Census Actuary, for 1971–93 on the basis of sample registration system conducted by Office of the Registrar General.

Note: *Life expectancy from 1981 onwards projected on basis of the 1981 Census by taking sex ratio as 105 males to 100 females.

improvement to about 60 years by 1990 in India. This occurred despite the substantive failure to reach health services to villages where seven-tenths of the population lived; had there been the implementation of a more progressive rural health policy there is little doubt that infant and under-five mortality rates would have reached their low 1990 levels much earlier.

In the period of independence from direct colonial rule, most developing countries privileged food production for their own population. The attempt to follow an autonomous trajectory of development also implied sharply growing wage-goods needs of industrialisation and hence the necessity of raising foodgrains output at a commensurate rate to avoid high inflationary pressures which would have adverse income distribution effects. Since the agricultural sector is the main source of economic surplus, various ways of extracting the surplus for industrialisation through taxation and through the terms of trade were thought to be necessary in the absence of a substantial expansion of trade and capital inflows. Most planned (China) and semi-planned, *dirigiste* economies (India) tried to avoid rapid inflation in foodgrain prices relative to manufactured goods prices, with varying success; while taxation of agriculture was conspicuously more successful in China after the formation of cooperatives, compared to India.

The developing countries which have a high proportion of poor in their population, are typically characterised by a high death rate as well as a high birth rate, with the birth rate substantially exceeding the death rate. The rate of natural increase which is the difference between the birth rate and the death rate, has to be further adjusted to obtain the actual increase of population by taking account of net migration. In the course of development, as health services reach a larger segment of the population, education levels improve and per head real incomes rise, it is expected that not only will the death rate come down but so will the birth rate. The aim is to obtain an even faster decline in the birth rate than in the death rate, if the rate of natural increase is to decline from the initial high levels.

In a class-divided society, average figures do not give a true picture of how bad the situation is for the most deprived, who owing to endemic lack of adequate nourishment are more vulnerable to sickness or morbidity. We know that poverty, under-nutrition and lack of access to affordable medical services get reflected in a much higher death rate than the average, for the poorer segment of the population. By the same logic, a given decline in the average death rate over time in the overall population, may well be very unequally distributed, with a very large decline in the already lower-than-average death rate among those in urban areas as their access to health services improves further, and a very small or non-existent decline in the death rate among the poorest in rural areas with initial high levels of deaths. A decline in the average death rate may be quite compatible even with a rise in the death rate for some segments of the population. In this context it is significant that with the inception of economic reforms and sharp macroeconomic contraction during 1991–93, there was a recorded rise in the crude death rate by 1993 in a number of states in India as well as a rise in the infant mortality rate (see Table 2.2).

Table 2.2
India: Statewise IMR in 1991 and 1993 and Vital Rates 1993

State	Infant Mortality Rate				Per cent Change 1993/1991	Death Rate 1993	Life Expectancy Years
	R 1991	U 1991	Combined 1991	Combined 1993			
Andhra Pradesh	77	56	73	66	9.6	8.3	60.6
Assam	83	42	81	77	4.9	9.6	54.9
Bihar	71	46	69	73	5.8	10.5	58.5
Gujarat	73	57	69	62	10.1	7.6	60.1
Haryana	73	57	69	68	1.4	8.0	62.9
Himachal Pradesh	76	38	75	n.a.	n.a.	n.a.	n.a.
Karnataka	87	47	77	62	19.5	7.6	61.9
Kerala	17	16	16	16	0.0	6.0	72.0
Madhya Pradesh	125	74	117	99	15.4	11.1	54.0
Maharashtra	69	38	60	55	8.3	7.4	64.2
Orissa	129	71	124	103	16.9	10.8	55.5
Punjab	58	40	53	54	1.9	7.3	66.4
Rajasthan	84	50	79	85	7.6	9.1	58.0
Tamilnadu	65	42	57	56	1.8	7.9	62.4
Uttar Pradesh	102	74	97	86	11.3	10.4	55.9
West Bengal	76	47	71	59	15.2	7.7	61.5
All-India	87	53	80	74	7.5	9.0	59.4

Source: *Health Information of India 1994* for first three columns, and *Economic Survey 1996-97*, Table 10.3.

Note: Bold type indicates states where combined (rural plus urban) mortality rate (the IMR, or the crude death rate) has risen compared to 1991. Data for 1991 death rate is given in *Economic Survey 1995-96*, Table 10.4.

The difference between endemic high death rate among the (mainly rural) poor, and what is identified by most academics as 'famine deaths', seems to be the fact that the first involves the poor dying at a rate higher than the average for the population, but slowly, unobtrusively and over a longer period of time owing to being chronically under-nourished and therefore being subject to higher morbidity; this higher than average death rate being considered nothing 'out of the way' or abnormal, given the existing unequal distribution of incomes which is taken as given. The second, which is considered not normal or usual and is termed 'famine deaths', involves a sudden rise in nutrition-deprivation and hence sudden rise in morbidity and death rate, usually among segments of the very same classes which are poor as a 'normal' state of affairs. In short, a sudden upward deviation from the prevalent death rate is thought of as 'famine death'. The cause of a sudden and unpredicted rise may be various—a sudden decline in output, which affects the already poor more than others, or a sudden rise in the price of the basic food staple relative to the income of the consumers. The second cause is not necessarily predicated on the first; a sudden rise in food price may take place, not because output is less than usual, but because government follows policies of suddenly increased expenditures, which rapidly expand incomes in the hands of one segment of the population and hence their demand for food, while leaving untouched the nominal purchasing power of another segment of the population, as happened

during the 1943 Bengal famine when deficit-financed defence-related expenditures were suddenly raised.

The Question of 30 Million 'Famine Deaths' during the Great Leap Period in China

China has performed much better than has India in reducing inequalities and reaching health care to its vast rural population and thereby reducing infant, child and maternal mortality rates as well as the overall death rate much faster than India has, over a comparable period (see Byres and Nolan, 1976; Liu Zheng, 1984). It is widely believed however that China had a more severe famine than India ever had, during the 'Great Leap' period, especially 1959–61 in which millions died; the figure of a total of 27 to 30 million famine deaths is frequently quoted. Over time, the lower estimate has dropped out and the figure appears to have become firmly fixed at '30 million' in the academic and in the general public mind. The main source of this figure is Amartya K. Sen's many writings and speeches which are more widely known and reported, certainly in India, than are the basic source which he uses, viz., the findings, embodied in a brief report in 1984, by the committee of demographers chaired by Ansley J. Coale (1984) with the aim of studying China's population trends following the 1982 Census in China, and the later more detailed study by Judith Banister (1987). The argument made by Amartya K. Sen is that the absence of democratic freedoms including press freedom in China explains the fact that public policy did not respond to social crisis and the world did not have any inkling that such a massive famine 'following the failure of the Great Leap Forward' had taken place at the time. In a recent book, *Development as Freedom*, the proposition is repeated by Sen in categorical terms.²

In general, the thrust of the argument is that collectivisation produces famine and the absence of a 'free' press as in capitalist countries, prevents anyone outside these countries from knowing about it until much later—when Western scholars painstakingly uncover the facts through their research. Since collective ownership and production is the very essence of socialist production relations, this appears to constitute a damning indictment of socialism. The issue is complicated by the fact that in China itself, some who were always opposed to egalitarian principles of distribution and wanted to dismantle the rural communes (which were indeed dismantled from 1980 onwards), have acquiesced in the argument of famine as an *ex post* justification for doing so, regardless of the fact that they themselves, despite their active involvement in political life were apparently quite ignorant at that time that such a massive famine with 27 to 30 million deaths, had taken place in their own country.

China has had population censuses at irregular intervals unlike the regular decennial census in India. For the first four decades, up to 1990, population census was carried out in China in the years 1953, 1964 and 1985. What we have done in Table 2.4 is to take the 1964 Census population total (see Table 2.3) as the reference point, and then calculated each preceding years' population by using the

official annual rates of natural increase, on the assumption that net migration was negligible. The resulting series which emerges gives a terminal figure for 1955 which is broadly consistent with the 1953 Census total as well. We have also reproduced the grain output series and grain procurement series from Riskin (1981). It is clear enough from this that, owing to a run of three bad harvest years, grain output in China did fall substantially, much more than it did in India during the its food crisis of 1965–67, and the death rate also rose. But the order of the rise is nowhere close to giving the 30 million figure over the three years 1959–61; in fact, a quick mental calculation of the excess death rate compared to the pre-famine year, applied to the population in each year, would give a number between one-third and two-fifths of that figure.

Table 2.3
Key Statistics on China's Population

<i>Census Date</i>	<i>Estimated Population</i>	<i>Growth Rate per Annum</i>	<i>Sex Ratio Males per '000 Females</i>	<i>Urban Population/ Total (%)</i>
1.7.1953	580.6	—	1075	12.9
1.7.1964	694.6	1.6	1055	19.1
1.7.1982	1008.2	2.1	1063	20.9
31.12.1985	1045.3	1.0	1069	36.6

Source: Visaria (1988), Liu Zheng, Song Jian and Others (1981).

Table 2.4
Vital Rates, Estimated Population of China, Grain Output and Procurement¹

<i>Year</i>	<i>Birth Rate</i>	<i>Death Rate</i>	<i>Rate of Natural Increase</i>	<i>Estimated Population (Million)</i>	<i>Grain Output (Million ton²)</i>	<i>Grain Procurement (Million ton)</i>
1955	32.60	12.28	20.32	594.05	183.9	19.7
1956	31.90	11.40	20.50	606.23	192.7	14.9
1957	34.03	10.80	23.23	620.32	195.0	17.4
1958	29.22	11.98	17.24	631.01	200.0	20.9
1959	24.78	14.59	10.19	637.44	170.0	28.0
1960	20.86	25.43	-4.57	634.53	143.5	21.5
1961	18.02	14.24	3.78	636.93	147.5	17.5
1962	37.01	10.02	26.99	654.12	160.0	16.1
1963	43.37	10.04	33.33	675.92	170.0	17.0
1964	39.14	11.50	27.64	694.60 ³	187.5	17.0
1965	37.88	9.50	28.38	714.31	194.5	17.3

Source: Liu Zheng, Song Jian and Others (1981), C. Riskin (1987: Table 6.5, p. 137).

Notes: ¹Vital rates are per thousand population.

²Grain output always includes tubers in China, converted to grain at the rate of 5 kg = 1 kg.

³1964 Census population figure for China, excluding Taiwan. This has been taken as the fixed point and the annual rates of natural increase, taken from C. Riskin (1981), have been applied to obtain the population series. Riskin's figures for birth rates, death rates and hence rates of natural increase reproduced above, are found to be identical with rounded values for these variables presented, not as continuous time series but in several separate tables, in Liu Zheng, Song Jian and Others (1981). The specific years 1960 and 1961 are however excluded from all tables in the latter book.

It would be instructive, therefore, to investigate how exactly the estimate of massive 'famine deaths' totalling 30 million, has been arrived at. But to anticipate, when we look carefully at the data we find that in fact, over 18 million out of the 30 million Chinese alleged to have died owing to famine, were not born in the first place. The Chinese are an extremely talented people and have moved mountains; but even their ingenuity would be stretched, we believe, to achieve the feat of dying without first being born. There is a basic responsibility that everyone, but more particularly academics have, to be clear and precise in their propositions. To say or write that '30 million people died in the famine in China' conveys to the listener or reader the picture of people who were actually alive, starving to death. This would represent an extremely large order of mortality, unprecedented in the history of developing countries even in the colonial period in the twentieth century.

The fact is, however, that it did not happen: 30 million people actually alive in China in 1958 did not die over the next three years. The proposition that they did, is a false one. There was indeed famine in the sense of substantial excess mortality compared to 1958 (which was a good agricultural output year), during the three bad agricultural years 1959 to 1961 when grain output declined by nearly one-third; we estimate in Table 2.6 that the cumulative extent of excess mortality owing to rise in the death rate compared to the immediately preceding normal year, was under two-fifths of the figure quoted, viz., is likely to have been of the order of 11.6 million over the three years, provided the death rates are not overestimated. (The fact that even the official death rates may be overestimated is suggested by the fairly adequate level of per capita food grain output even in the worst year, 1960 and even after procurement is deducted, a question discussed at the end of this paper.)

Even if we consider A.J. Coale's much higher death rates, constructed as we shall see on dubious assumptions, the excess mortality over the three years with 1958 as the benchmark works out to 13.7 million (Table 2.7), less than half the quoted figure. Accepting the official death rates for the time being, a famine involving 11.6 million excess deaths of actually living people is quite bad enough; academics have a responsibility not to give currency to sensationalised estimates of more than two-and-a-half times higher deaths as has been done, most unfortunately, and done repeatedly. It is like saying that 8 million people died in the Bengal famine of 1943, when all data show actual excess mortality of the order of 3 million or so.

The way that the inflated estimate for 27 to 30 million deaths in China has been arrived at, is through two alternative routes: first, to take not only the actual excess mortality on the basis of the available information on the rise of the death rate, but to add on to it the estimated numbers of babies not born at all, owing to a fall in the birth rate which was steep during this period. What is being done here is to estimate the number of 'missing millions' in the population pyramid for these years, but this is misleadingly designated as 'famine deaths'. Whether we take the official figures or Coale's constructed figures, this procedure gives the total figure of a 27 million deficit in the population by 1961 compared to 1958, but over three-fifths of the deficit in both cases arises from fall in the birth rate and does not represent actually living people dying during the famine (see Tables 2.6 and 2.9).

It cannot be validly assumed either that the entire fall in the birth rate was necessarily food-shortage induced, for the birth rate fall was already marked in a good grain output year, 1958 compared to 1957, and there were important institutional changes during 1958 to 1961 which are likely to have affected reproductive behaviour and tended to lower fertility, over and above the usual adverse effects on fecundity of falling nutrition during the subsequent output decline during 1959–61.

The second route yielding the '27 to 30 million famine deaths' claim is also highly dubious at the methodological level in involving statistical procedures which are opaque or inadequately justified. This route has been followed by the US demographers and it consists of statistically reconstructing entirely new series of birth rates and death rates for the inter-censal period 1953 to 1964 on the basis of the fertility survey in the much later 1982 Census. This statistical reconstruction involves assumptions carrying a substantial degrees of arbitrariness, and the results, while jacking up all vital rates before 1971, jacks up to a proportionately higher degree the death rates in the very early 1950s and in 1960, to levels much higher than seem to be consistent with either grain output figures in those years, or with the vital rates survey of 1953. Deaths from these dubiously reconstructed death rates are then subjected to linear trend fitting (despite the fact that death rate behaviour is necessarily non-linear) and the excess of all assumedly higher deaths during the shortage years above this fitted linear trend is called 'excess deaths' (which is 27 million according to Coale and 30 million according to Banister) and assigned to the 'famine.'

We will look at this procedure again in more detail later, suffice it to point out that while unlike in the first route, unborn babies during 1959–61 are not being included here, what is being included is *hypothetical* steeply higher death rates for 1959–61, and an estimate of 'excess deaths' which is effectively (owing to linear trend fitting) based on the assumption that the death rate should have continued to decline, at a rate influenced by the hypothetical steeper rate of decline assigned to it for the 1950s. This procedure thus involves not only reconstructing death rates but also an implicit and methodologically dubious definition of 'excess' deaths by projecting falling death rates, which is a definition which does not seem to have been ever applied by demographers and economists before, and never applied in contexts other than China.³

What is the alternative measure of excess deaths that we can use, in place of the non-transparent and methodologically dubious procedure summarised above? A clue is provided by the fact that, even after a period of decline in death rate, people are not inclined to interpret a stable death rate for a few succeeding years as 'excess deaths' compared to the earlier period. In short, we consider that an 'excess' cannot be defined in relation to some *hypothetical* decline which might have taken place under different circumstances. An 'excess' can only be defined as a rise in relation to a benchmark level. India, for example, has had high, very slowly declining death rates (its normal 1960 death rate of 24.6 was little different from China's abnormal official famine death rate of 25.4 in that year) without this being designated as 'excess deaths' leave alone 'famine deaths' in relation to the hypothetical much lower death rate it could have achieved with alternative policies. The same argument applies to other relevant variables: for example the rate

of decline of the IMR has markedly slowed down in the economic reforms period in the 1990s in India compared to the 1980s, and while this is to be noted and is a cause for concern, we do not designate as 'excess deaths' the difference between actual infant mortality in the 1990s and the trend inclusive of earlier faster decline. 'Excess deaths' would apply to an actual rise in mortality in any sensible definition—a rise such as is observed for some states in India between 1991 and 1993 (see Table 2.2).

When there is an upwards spurt in the death rate induced by output shortfall or by other causes in a particular year or years, the conceptually simple and transparent measure, understood by all, is to estimate the excess mortality by applying the normal mortality rate in a preceding normal, benchmark output year and look at the difference made to the population by the actual, higher mortality. This is the procedure we have adopted in Tables 2.6 and 2.9.

Conceptually, it is the excess of actual mortality in a particular year over the benchmark of a normal year, which is thus the transparent and correct measure of 'famine deaths'. Of the alleged 27 million famine deaths during 1959–61, as mentioned more than three-fifths were not excess of deaths but deficit of births, owing to fall in the birth rate. There is a world of difference between people not being born at all, and actually living people dying of starvation or disease during a famine. The second may be legitimately referred to as 'famine deaths' but not the first, for one has to be alive in the first place to suffer death. While it is true that there is a causal link between nutrition and fertility in that periods of low nutrition are also those of low fertility, there are many factors other than nutrition which can affect the birth rate, and the rapid institutional changes taking place during the Great Leap for capital formation purposes (involving rural labour mobilisation, communal kitchens, etc.), which disrupted family life, were precisely such that the birth rate might be expected to fall to some extent, quite independently of what was happening to nutrition levels. This is also supported by the fact that the birth rate in China apparently rebounded and overshot at a far faster rate than output did as some institutional changes were reversed; between 1962 and 1965 excess births (continuing to use the 1958 birth rate as the benchmark) totalled 27.7 million, more than making up for the earlier population deficit during 1959–61 while reduction in the death rate relative to 1958, also contributed additional population (see Table 2.6).

Finally, postulated trends in any set of data or results, should be checked for consistency with values of other known important macroeconomic variables and their trends. The share of the rural poor in the rural population in India, for instance has been shown in various econometric exercises to vary inversely with per capita food production and directly with food price, with variations in rural public expenditure playing also an important explanatory role through employment generation (see Ravallion and Datt, 1995; Sen, 1996; Sen and Patnaik, 1997). In China food prices were strictly controlled administered prices, while employment generation was always privileged and days employed per worker rose steadily before 1980,⁴ so the main independent variable affecting variations in poverty becomes the state of the harvest.

Looking at the per capita output of foodgrains in China in the worst years of output decline, compared to the years of lesser crisis in India, we find that Chinese per head availability from domestic output alone still remained noticeably *higher* than availability including imports in India, which did not suffer any generalised famine as China is alleged to have done. Given the more egalitarian nature of Chinese society compared to India's by that date, in the light of the fairly adequate levels of per head food output in the worst crisis years in China (higher than India's in the 1960s, and higher when compared to 1987–88, India's last drought year), even the official order of rise in the death rate in 1960 to 25.4 per thousand, seems quite surprising.

The much higher death rate figures, which are figures which moreover diverge from each other as well, which have been constructed inferentially on various more or less arbitrary assumptions by Coale and by Banister, in particular the death rate for 1960, seem to be quite improbable. While Coale (1984) has no reference at all to food output trends in the brief report of the committee he chaired, Banister (1987) in her much more detailed book, shows an uneasy awareness of the inconsistency of her derived figures of death rate and birth rate, with the firm food output data. But instead of tackling the problem head on, or modifying her apocalyptically high constructed 1960 death rate, she relegates comment on the inconsistency to a long unnumbered footnote on page 233. But this inconsistency is the very crux of the matter, which seriously undermines the credibility of both Coale and Banister's rewriting of China's demographic history.

In the section below let us take up these questions one by one, starting with an estimate of the excess mortality and deficit of births implicit in the Chinese official statistics quoted in Liu Zheng, Song Jian and others (1981), and in Riskin (1981).

An Estimate of the Order of Excess Deaths and Deficit Births

In Table 2.4 we reproduce the vital rates for China as given in C. Riskin (1981). *China's Political Economy—the Search for Development since 1949*, after cross-checking these rates against the China Studies Series publication from Beijing, *China's Population: Problems and Prospects* by Liu Zheng, Song Jian and Others (1981). We find that while the vital rates and rate of natural increase for different years agree in the two sources, there is some inconsistency of the absolute population figures given by the Chinese scholars, with their own rates of natural increase. Accordingly we have worked out the population series implied by their rates of natural increase, taking as the fixed point the 1964 population estimate of 694.6 million (see Table 2.4).

The estimated death rates for China at the time of Liberation vary quite widely, ranging from 28 to much higher levels of 35 and more. The initial rate of 28 for China is virtually identical with the 1941–51 inter-censal death rate of 26 to 30 in

India. After the redistributive land reforms of the early 1950s which substantially did away with class inequalities, there were systematic mass drives for cleaning up the environment and eradicating disease-carrying pests, while with the formation of rural cooperatives from 1954, there was the inception of a rudimentary rural health care system. The emphasis was on public hygiene and preventive rather than curative medicine; in a poor, largely rural society this is likely to give good results. By 1958, the death rate in China is officially estimated to have declined to below 13 per thousand. (Coale, 1984 and Banister, 1987 place their constructed death rates using the 1982 Census data and assuming under-reporting, much higher with the range between 18 and 21 for 1957–58, which if true would still represent a considerable achievement for China since India did not reach this level until nearly two decades later.)

Looking at the years 1959 to 1961 in Table 2.4, we find that the death rate rose in 1959 by 2.6 per thousand compared to 1958, and rose further sharply in 1960, by as much as 10.8 per thousand over that of 1959, reaching over 25 per thousand. While it declined as sharply in the next year it was still 2.2 per thousand higher than in 1958. There is no doubt that the 1960 rise was highly abnormal, and the reason, as Riskin (1981) plausibly argues, lay not only in the nearly 30 per cent output decline owing to the second successive bad agricultural year; the shortfall's effects were aggravated by the excessive procurement burden placed on the peasantry, which reduced retained output in their hands more severely than gross output was reduced.

Taking 1958 as the benchmark year, there were excess deaths during 1959–61 totalling 11.6 million, the largest impact being in 1960 when excess mortality was over 8 million. Because China in the single preceding decade before 1959 had reduced its death rate at a much faster rate (from 29 to 12 comparing 1949 and 1958) than India had, this sharp rise to 25.4 in 1960 in China still meant that this abnormal, famine death rate was virtually the same as the prevalent death rate in India which was 24.6 per thousand in 1960, only 0.8 lower. This latter rate was considered quite 'normal' for India; further, in both the preceding and the succeeding year India's crude death rate was 8 to 10 per thousand *higher* than in China. Of course, each economy has to be judged in relation to its own internal performance; and no doubt the rise in the death rate during the worst years of output shortfall is a bad blot for China on its otherwise very impressive record of rapid decline and good food security. But it is not correct to say that 'famine deaths' totalled as much as 30 million; and it is not correct to imply that absence of press freedom meant that China's then leaders, despite knowing about such massive deaths, were so cynical and depraved that they could mislead the world successfully.

If China had continued to have the same birth and death rates as in 1958, the normal benchmark year, then its rate of natural increase would have remained at 17.4 per thousand. Applying this rate, the projected population for China in 1961 would have been about 27.3 million higher than it actually was. Thus the population deficit during the three abnormal years turns out to be 27.3 million (Table 2.5). By a slightly different measure, taking the 1958 benchmark, the excess of

deaths as mentioned totals 11.6 million and the deficit of births totals 15.3 million (Table 2.6). We have also estimated the population series implied in Coale's 'roughly adjusted' vital rates and we find that the projected population in 1961 using his data compared to the actual estimated population, turns out to be 37.98 million less (see Table 2.8). The steeper rise in death rate and much steeper fall in birth rates assumed in Coale's adjusted series leads to a difference with respect to estimated excess of deaths and deficit of births. As Table 2.9 using Coale's adjusted rates shows, the excess of deaths is 13.7 million and the deficit of births is as much as 23.7 million during 1959 to 1961. In other words, if we apply the conceptually transparent and correct measure of excess deaths to Coale's own series, then we obtain an estimate of 13.7 million excess deaths. This is likely to be a substantial overestimate because the constructed death rate itself is an overestimate and, as we shall see, is also inconsistent with food output levels.

Table 2.5
Projected Population in 1961 using the 1958 Rate of Natural Increase

Rate of Natural Increase in 1958: 17.24 per thousand and Population	631.01 million
Projected population by 1961 @ 17.24/1000 RNI	664.212 million
Actual population in 1961	636.925 million
Deficit of population	27.287 million

Source: Calculated from the data in Table 2.4.

Table 2.6
Excess/Deficit of Births and Excess/Deficit of Deaths, 1959-61, and 1962-65, Assuming 1958 as Base Rates

1959-61 Base: 1958, with birth rate (29.22) and death rate (11.98)

<i>Year</i>	<i>Deficit of Births</i>	<i>Excess of Deaths</i>	<i>Sum</i>
1959	2.8306	1.6637	4.4943
1960	5.3047	8.5344	13.8391
1961	7.1336	1.4395	8.5731
Total	15.2689	11.6376	26.9065

1962-65 Base: 1958

<i>Year</i>	<i>Excess of Births</i>	<i>Deficit of Deaths</i>	<i>Sum</i>
1962	5.0956	1.3113	6.4069
1963	9.5643	1.2242	10.7885
1964	6.8904	0.3334	7.2238
1965	6.1859	1.7715	7.9574
Total	27.7362	4.6404	32.3766

Source: Calculated from Table 2.4 by applying the difference between the death rate or birth rate in a specified year and the corresponding rate for 1958 treated as base year, to the specified year's population.

Note: The sum of annual deficit of births and excess of deaths will not add up to the population deficit estimated in Table 2.5, because the population base in this table is altering in successive years compared to Table 2.5.

Table 2.7
A.J. Coale's 'Roughly Corrected' Vital Rates

<i>Year</i>	<i>Death Rate</i>	<i>Birth Rate</i>	<i>RNI</i>	<i>Estimated Population¹</i>
1954	29.1	44.4	15.3	589.28
1955	22.4	41.3	18.9	600.42
1956	20.8	40.2	19.4	612.06
1957	19.0	41.1	22.1	625.59
1958	20.4	37.7	17.3	636.41
1959	23.3	28.3	5.0	639.60
1960	38.8	25.2	-13.6	630.90
1961	20.5	22.3	1.8	632.03
1962	13.7	40.9	27.2	649.22
1963	13.0	47.3	34.3	671.49
1964	13.5	40.7	27.2	689.75
1965	11.1	39.7	28.6	709.48
1966	10.4	38.3	27.9	729.28

Source: Coale, 1984: Table 5, p. 47 and Table 10, p. 69.

Note: ¹This series of estimated population is not in Coale. It has been calculated by us using his adjusted vital rates and taking the 1953 Population Census figure of 580.4 for mainland China as base.

Table 2.8
Population Deficit, 1958-61

Projected Population @ 17.3/1000 (1958 RNI) by 1961:	670.01
Actual Estimated Population in 1961	632.03
Population Deficit	37.98

Source: Calculated from Table 2.7 data.

Table 2.9
Excess/Deficit of Births and Deficit/Excess of Deaths, 1959-64

<i>Year</i>	<i>Excess of Deaths</i>	<i>Deficit of Births</i>	<i>Year</i>	<i>Deficit of Deaths</i>	<i>Excess of Births</i>
1959	1.85	5.98	1962	4.48	2.11
1960	11.77	7.88	1963	5.05	6.55
1961	0.06	9.88	1964	4.83	2.10
All	13.68	23.74	—	14.36	10.76

Source: Table 2.7 data with 1958 as base. Note to Table 2.6, applies here as well.

The first route of estimating famine deaths in China by taking it as identical with the deficit of population by 1961, is clearly wrong. The 'deficit of population' is a valid enough measure provided it is termed as such; what makes for tendentiousness is to say that 27 million people 'died in the famine' which clearly, and wrongly suggests to the unwary reader and non-expert that they all actually existed to begin with.

Periods of food shortage do lead to decline in fertility; this is a fairly well established proposition. Periods of mass mobilisation of males, for military service for example, also get reflected in a decline in the birth rate. There was no

military conscription at this date in peacetime China, but there was something equivalent affecting both males and females in the villages. There was a massive mobilisation of both male and female workers for a stupendous construction effort during this period of early commune formation. Able-bodied women were drawn from the domestic sphere into construction work. The established peasant family living and work patterns were radically re-organised with the formation of the communes. Large bands of men and women set out in teams and brigades for constructing water management systems, cleaning up the environment and eradicating disease-carrying organisms, afforesting hills, terracing and bunding and so on. The private plot was abolished temporarily to facilitate mobilisation on a virtual war footing. Workers including women spent weeks on the work-sites, and there were communal kitchens and creches to look after children. (A detailed account at the village level may be found in W. Hinton's *Shenfan* [1984]). It is not surprising if this comprehensive disruption of normal peasant family life in the interests of construction, also contributed greatly to the observed decline in the birth rate, as birth decisions were postponed and in any case physically rendered more difficult. Abolition of private plots and insistence on communal kitchens were soon attacked as 'ultra-left' measures as output declined in 1959. With the stabilisation of the new system, there was a dismantling of communal kitchens and reversal to family life. The birth rate again surged to unprecedented heights, from 1962 onwards peaking at 37.9 in 1964, as though there was a pent-up demand for children which was now being given full play.

It is inconceivable that such a large number of 'famine deaths' as even the realistic 11.6 million should have been wilfully suppressed by a state in China which had demonstrated its commitment to peoples' welfare by undertaking measures to reach basic food security and health services to the poor, and which had achieved a much faster reduction in infant mortality and the death rate in the very first decade of independence than had India. We may try to provide a hypothesis regarding why no-one including the Westerners in China, even noticed that mortality was higher during these years.

As regards the genuine excess mortality during China's difficult famine years, while shortages and deficiency diseases were very real and visible, massive famine appears to have been near invisible to all including the foreign diplomats and journalists present at that time in China. Even though there was famine, we may hypothesise that its precise dimensions may have been, paradoxically, rendered opaque by the substantially egalitarian structure of Chinese society which was no longer characterised by sharp class differences. The food shortage was not concentrated in a sharp drop in consumption by the members of a particular deprived classes like poorer peasants and labourers who then migrated to cities and died in the sight of all, while others had more than enough to eat, as typically happens with famine in class societies, and as happened for example in the great Bengal famine of 1943-44. Food shortage was spread out over the rural consuming population much more evenly and therefore must have led to higher rates, but not immediately or obviously visible higher rates of mortality for particular groups. We would also expect the shortages to have led to higher mortality in the particularly

vulnerable segments in an otherwise equal society—parturient mothers, infants and the very old. It is a mistake to think that all real trends in vital indicators are necessarily visible to the individuals at the time.

Thus even though we ourselves in India have lived through the period when infant mortality has fallen greatly, it is a matter we are convinced of not from our direct experience of it, but after the numbers have been counted and presented to us. China's leaders were not guilty of wilful suppression of knowledge of the higher mortality; the knowledge itself was built up much later than the events, and an approximation to the correct estimate as we have seen is just over one-third of the sensationalised estimates which are still being circulated.⁵

Let us now turn to the second route through which massive famine deaths are estimated, namely by re-writing the death rates and birth rates in China for the two decades after Liberation by substantially raising both, and then fitting linear trends. This is the exercise that a number of US demographers have been carrying out, starting with the committee with Ansley J. Coale in the chair, whose report on *Rapid Population Change in China 1952–1982* became available in 1984 while Judith Banister published her book in 1987, based on following much the same method as Coale (1984) but arriving at even more apocalyptic findings. It is important to study how exactly they have arrived at their results regarding 27 to 30 million 'excess deaths' since none of those who freely throw the 30 million figure around seem have looked at the original procedure of arriving at it.

First, high fertility figures were obtained from a large sub-sample (0.1 per cent of the population) in the 1982 Census, which were then applied to work backwards and obtain a completely new constructed series of births starting from thirty years or more earlier (from 1953 to 1981 in Coale and from 1949 to 1984 in Banister), which were found to exceed the estimated births in the official series. According to Nai Ruenn Chen's useful book (Nai-Ruenn Chen, 1966: 6), a very large survey of 30.18 million persons had been carried out in addition to the 1953 Census, specifically to ascertain vital rates. This was a 5 per cent sample of the population and yielded a birth rate of 37 and death rate of 20; but these findings are brushed aside by the US demographers who reconstruct much higher birth rates—in the 40s—and thereby, much higher death rates—in the high 20s—for the first half of the 1950s). Total reliance was thus placed by them on the Census 1982 responses to questions about births, even though this showed improbably high fertility rates for the late 1950s and 1960s compared to similar societies like India;⁶ and even though biases might have been introduced by the one-child policy already in force for a decade (by inducing the possible backward shifting of reported births to years before 1970).

If extra people were born, some of those extra people must have died. The difference between the constructed total of births over the inter-censal period 1953–64, and the official inter-censal increase in population, is treated as the actual total of deaths, which exceeds the official total by a heroic 60 per cent. However, it is not known *when* these assumed extra deaths took place. These assumed extra deaths are then allocated arbitrarily over the inter-censal years, in such a manner that the official death rates are adjusted upwards by a significantly

higher proportion in the early 1950s and during 1958–60, than in other years (Coale, pp. 30, 66–68; Banister, pp. 114–15). All these statistical manipulations, require total faith in the accuracy of the official Census population totals, even while every official vital rate is assumed to be underestimated.

The element of arbitrariness thus enters at two points, first in the rejection of all other official data even from very large samples, but acceptance of 1982 fertility data; to rework birth rates for even thirty years earlier; and the necessarily arbitrary allocation of the resulting assumed under-reported deaths to the various inter-censal years. Both A.J. Coale (1984) and J. Banister (1987) follow this procedure, and since neither gives any results of making alternative assumptions regarding assumed under-reporting from the ones they have actually made, in order to allocate the extra deaths to particular years, the sensitivity of their results to their specific assumptions remains unknown to the reader. (In fact, a quick calculation shows that the constructed death rate in a particular year is highly sensitive to the assumed degree of under-reporting and that a slightly different assumption would make a large difference.)

Because of their manner of calculating and allocating the derived extra deaths, both therefore obtain (a) much higher values of death rates and birth rates during 1950 to 1961 than in the official data, higher by 8 to 22 per 1000; (b) a steeper rate of fall—in Coale but not in Banister—in the death rate up to 1958 compared to official data; and (c) a steeper rate of rise in the death rate from 1958 to 1960. The arbitrariness of the procedure is illustrated by the variation between the two estimates themselves: Coale has the death rate declining by 30 per cent during 1954 to 1957 while Banister has it declining by 15 per cent. Coale has the death rate rising by 90 per cent between 1958 and 1960, from 20 to nearly 39 (see Table 2.7) while Banister has it rising by 118.4 per cent over the same period from 20 to 44.6 (see Table 2.10). Coale has a 1960 death rate which is nearly 39 compared to the official 25.4 while Banister goes one better, making assumptions which hike the 1960 death rate to 44.6 (Each one-point rise in the imputed death rate for 1960, automatically adds 0.63 million to the estimated excess deaths in that year alone, even following the bench mark method).

The reader no longer knows what the death rate in 1960 in China was. A variation from 25.4 (official value) to 44.6 (Banister) is no mean variation. The 1960 death rate becomes like a hot-air balloon which drifts higher and higher with the degree of stoking and the heat generated below it. With appropriately different assumptions about under-reporting in that year the rate could be hiked up further: why not to 50, or even more? With alternative, equally plausible or more plausible assumptions than those made by Coale and Banister, one could also reconstruct the death rate in 1960 to a level lower than the official one of 25.4. There is absolutely no reason to accept the Coale or Banister reconstruction rather than any other; indeed their reconstruction comes into patent conflict with the output data at several points. Coale's reconstructed death rate of 29 for 1954, which is his peak death rate for the entire 1950s decade, becomes meaningless when we see that 1954 was a normal year when grain output was 2.2 per cent higher than in 1953 and there was absolutely no reason for the death rate for the 1950s to have peaked in that year.

Table 2.10
Banister's Constructed Vital Rates, 1953-65

Year	Death Rate	Birth Rate	Rate of Natural Increase
1953	25.8	42.2	16.4
1954	24.2	43.4	19.2
1955	22.3	43.0	20.7
1956	20.1	39.9	19.8
1957	18.1	43.3	25.2
1958	20.7	37.8	17.1
1959	22.1	28.5	6.4
1960	44.6	26.8	-17.8
1961	23.0	22.4	-0.6
1962	14.0	41.0	27.0
1963	13.8	49.8	36.0
1964	12.5	40.3	27.8
1965	11.6	39.0	27.4

Source: Birth rate using 1982 fertility survey, and reconstructed death rate (Banister, 1987: 352, Table 10.1).

Banister recognises reluctantly in a footnote (Banister, 1987: 233) that 'there is some discrepancy between China's grain production and the apparent timing of the famine.' Banister's assumption that the famine started early in 1958 because the birth rate starts dipping in that year, even though output was normal, is quite unwarranted and is the result of ignoring all factors other than nutrition which might have affected fertility (we have already indicated such factors as drastic change in living and working habits during collective labour mobilisation at the inception of the communes, so well described by William Hinton at the village level in *Shenfan*).

There is a large discrepancy with regard to output trends and Banister's story on fertility. As Banister herself recognises (p. 233), fertility is expected to decline nine months after the worst nutritional stress, whereas according to her own reconstruction, it declined much more in 1959 over the previous year (by as much as over nine per 1000) though 1958 was a good harvest year, than it did in 1960 over 1959 (less than two), or in 1961 over 1960 (just over four: see Table 2.10). She is unable to explain this. In fact, her rigidly held assumption that the initial large decline in the birth rate was necessarily only because of lower fecundity from lower food intake, itself is a wrong assumption and arises from the evident lack of any knowledge of or insights into the processes of profound changes in conditions of peasant collective work taking place at that time, which would necessarily have impacted on the birth rate.

To convert underemployed labour into capital assets, especially irrigation assets, was at the core of the massive labour mobilisation strategy of the Great Leap involving commune formation. This in itself was both conceptually bold and correct in terms of economic logic as a number of scholars have recognised—some 16.5 million *mu* was added to irrigated area during 1958-61 apart from other asset creation.⁷ In actual implementation there were problems because, first, there

appears to have been excessive initial centralisation and hence arbitrariness, with the entire huge commune as the unit of decision making and accounting, rather than the brigade (village) as was the case later; second, there was drastic overestimation of immediate output gains, without taking account of the fact that all capital formation projects involve lags before yielding returns, and therefore there was excessive procurement arising from inflated output estimates;⁸ and finally the entire enormous exercise in labour mobilisation through institutional change, happened to coincide with a run of bad harvest years. Once these problems were recognised and rectified and the commune system had settled down however, from 1962 onwards it seems to have generated a very impressive order of tangible capital formation and employment both on farm, and in off-farm rural enterprises (see Brammal, 1993; Rawski, 1979, 1982; Wheelwright and McFarlane, 1970), leading to a substantial rise in the real standard of living up to 1978, which was contributed to by greatly improved rural health and primary education systems set up out of collective funds.⁹

Returning to the estimates of famine deaths, the next and equally dubious part of the reconstruction exercise in Coale (1984: 7, 69–70), is to fit a linear trend to the constructed deaths for 1953 and 1964, obtained by applying the initial imputed much higher death rate and later imputed steeper rise, and then to say that the entire difference between the linear trend and constructed deaths for particular years represents excess deaths (27 million in Coale). Banister does not specify what procedure she has followed but merely informs the reader that 'the computer reconstruction of China's population trends utilised in this book, *which assumes under-reporting of deaths in 1957 as well as in all the famine years*, results in an estimated 30 million excess deaths during 1958–61' (Banister, 1987: 85; emphasis added). The computer will work out the procedure fed into it, and from the similarity to Coale's result we may perhaps infer linear trend fitting in this case as well.

Why anyone should at all go about fitting linear trends to deaths derived from a variable, the death rate, which is known to behave necessarily in a non-linear manner (declining and then bottoming out), is not at all clear. In fact, it is a wrong procedure, involving the implicit assumption that the death rate should continue to decline, at a rate influenced by the extremely steep initial decline implicit in the constructed series for 1953–58. But this is impossible; we know that the death rate in a population can never go on declining to hit a value of zero; not even the Chinese have found a way of ensuring immortality.

Let us consider Coale's linear trend formed, as he tells us on page 70, by using hypothetical deaths for 1953 to 1964 (10.4 million and 9.4 million). On projecting this linear trend the reader finds it should have hit the x-axis around 1990 by which time the Chinese population should already have zero deaths and have attained immortality (see Coale, 1984: p. 7, Graph 2.1 on which these values may be plotted by redefining the y-axis as total deaths). In Banister's similarly reconstructed graph (Banister, 1987: p. 353, Figure 10.1) the linear trend of deaths (15 million and 6.7 million) would when projected reach the zero deaths level even earlier, by 1977.

If this procedure of projecting a linear trend is patently absurd in the case of a variable like deaths, so is the actually undertaken procedure by Coale and Banister

quite meaningless, of fitting a linear trend to hypothetical deaths for positive values of death rate, and calling the difference of the hypothetical values for particular years from this trend, as 'excess deaths'.

Inconsistency of Assumed High Mortality with Food Availability

The very high death rates 'constructed' by Coale and Banister for the early 1950s and for 1960 (as we have seen, for the latter year, nearly 39 in Coale and as much as 44.6 in Banister compared with 25.4 in official sources) also appear to be quite inconsistent with the known per capita foodgrains availability out of domestic output in China in 1960. Indeed even the much lower official death rate of 25.4 in 1960, needs to be explained in the light of the fact that the lowest possible calculation of per capita food output that we can make for China, gives us figures which are not only substantially higher than the per head output, but are also much higher than availability (including imports) in India during its period of food shortage and stress in 1965-66 and 1966-67, a few years after China's crisis, which produced no generalised famine. China's worst year was 1960. The output per head figure for that year, no matter how hard we try to obtain a minimal estimate, also remains stubbornly above the corresponding figure for a much later minor drought year, 1987-88 in India after two decades of green revolution.

China had a per capita grain output which was about one-third higher than India's even in 1951, and has steadily increased the margin in its favour, apart from 1959-61 when its output per head came closer to India's but still remained higher.¹⁰ Foodgrains output was substantially lower than normal for two successive years in India, 1965-66 and 1966-67, which did not experience famine, despite its highly inequalitarian rural structure. In Table 2.11 we have adjusted

Table 2.11
Per Capita Food Output for China (1958-61)

Year	CDR ¹	CBR ²	Total Population	Per Capita Gross Grain Output ³ (kg)	Per Capita Net ⁴ Grain Output (kg)
1958	20.4	37.7	631.01	278.9	300.9
1959	23.3	28.3	637.44	234.7	236.2
1960	38.8	25.2	634.53	199.0	203.9
1961	20.5	22.3	636.93	203.8	216.4

Source: Vital rates and Population from Table 2.4.

Notes: ¹CDR = Crude death rate.

²CBR = Crude birth rate.

³Gross grain output is output of grain and tubers, roughly adjusted for unhusked paddy by taking 0.88 times grain output given in Table 2.4. Per capita value is obtained by dividing this by total population.

⁴Net grain output is obtained by deducting procurement from output in Table 2.4, then taking 0.88 times the result. It is then divided through by rural population (assumed to be 83 per cent of the total on the basis of urbanisation data in Table 2.3) to give retained

China's grain output downwards by 12 per cent to take account of unhusked paddy in her food statistics compared to rice in India. Per capita output for China is on two bases: gross output, and net output, after deducting procurement. It is assumed that all procured output in China went to urban areas and no part was redistributed to food-deficit rural areas, so the output net of procurement is divided through by the rural population to obtain rural availability. Finally, potatoes have been included along with foodgrains in India following the Chinese practice, using the same conversion rate (5 kg potatoes equal to 1 kg grain) as in China (Table 2.12).

Table 2.12
India: Per Capita Food Output Availability, 1966-68, 1988

Year	Population (Million)	Gross Grain Output (mm. ton)	Per Capita Availability			Total ¹ (Inclusive of Tubers) (kg)
			Domestic Output (kg)	Imports (kg)	Total (kg)	
1966	493.2	71.1	144.2	20.9	165.1	166.4
1967	504.2	73.1	145.0	17.3	162.3	163.7
1988	799.2	132.9	166.3	3.5	169.8	173.3

Source: Calculated from *Economic Survey 1995-96, 1998-99*. Read 1966 as agricultural year 1965-66 and so on. All except last column, excludes tubers.

Note: ¹Grain output figures in India do not include tubers, so this has been added in the last column only, at the conversion rate employed in China, for comparability with figures in Table 2.11.

It may be seen that China's rural per head food output net of procurement, for 1960 was 57.5 kg higher than India's overall per head output (including tubers) without deducting procurement in the worst year 1966-67, and was 40 kg higher than India's availability (which included large net imports of 10 million ton or 17.3 kg per head). China's overall gross per head grain output in 1960 was more than 50 kg higher than India's corresponding 1966-67 output and 35.3 kg higher than India's availability in that year.¹¹ Given the egalitarian structure of rural society in China by this date, the fact of famine in China when no famine was experienced in India is certainly a major puzzle. After two decades of Green Revolution, India experienced a drought year in 1987-88, when per capita food output including tubers dipped to below 170 kg and availability to around 173 kg. There was no question of 'famine' in the sense of a general rise in the rural death rate even though at least two-fifths of the population was below the poverty line. China's per head gross output in 1960 turns out to be nearly 30 kg higher and per head net output, nearly 35 kg higher than in India in 1987-88. Why China should have experienced a famine at all in 1960 when its food output per head was so much higher than in India decades after the latter's Green Revolution, is a question which needs to be investigated and answered before it is possible to accept uncritically even the official Chinese demographic account for the years concerned.

1. The classic exposition was by R.C. Dutt (1970a and b) *The Economic History of India* (2 vols.) for a summary of historians' arguments, see Patnaik (1999), 'The Process of Commercialization under Colonial conditions' in *The Long Transition—Essays on Political Economy*.
2. It must, however, also be noted that there are real handicaps that China experiences compared with India because it lacks democratic freedoms. This is particularly so when it comes to flexibility of economic policy and responsiveness of public action to social crisis and unforeseen disasters. The most prominent contrast lies perhaps in the fact that China has had what is almost certainly the largest recorded famine in history (when thirty million people died in the famine that followed the failure of the Great Leap Forward in 1956–61), whereas India has not had a famine since independence in 1947 (Sen, 1999: 34).
3. We do not see these, or any other demographers or economists fitting linear trends by including the falling death rates in Russia between 1945 and 1990 when it was still a constituent part of the Soviet Union, and then estimating the excess deaths resulting from market-oriented 'shock therapy' in the 1990s, by adding up all the actual deaths above these trends. Between 1990 and 1994 alone, the death rate for able-bodied males of working age in Russia has shown a rise from 48.8 to 84.1 per thousand and expectancy of life at birth has fallen significantly.
4. For steady rise in employed days in rural areas, see Schran (1969) and Rawski (1979, 1982). A brief discussion of surplus labour mobilisation through the co-operatives and later communes is contained in Patnaik (1998).
5. On a visit to China in the eighties, at the time the inflated 'famine deaths' estimates were being talked about in the West, this author mentioned these estimates and asked some very senior Chinese economists involved in the planning process, about their own experience of this period. They were extremely surprised and said that while there were cases of many more deficiency diseases than usual they were not aware of widespread famine deaths.
6. The fertility rates from the 1982 Census in China gives TFR values well above 6.0 for many years in both the 1950s and the late 1960s and even 7 in one year. The TFR value has been below 6 in India from the second half of the 1950s and has steadily declined despite much poorer food and health security than in China.
7. Wheelwright and MacFarlane (1970), point out that 'Nurkse-type savings' by conversion of deferred-wage labour to capital was the core of the strategy. Hinton (1984: Chapters 29–33) documents the enthusiasm with which villagers spoke of the Great Leap despite all the problems. See also my discussion (1998) of the relation between capital formation through surplus labour mobilisation, deferred wages and egalitarianism.
8. Hinton (1983, Part III, gives a graphic account of how Long Bow village got some of the grain already procured, given back to it thanks to the insistence of a sensible local cadre that output had been overestimated, and thus avoided shortages faced by adjacent villages.
9. The leap in employment per worker and addition to farm productive assets up to 1975 documented by Rawski (1979, 1982) suggests that it was a successful strategy. During visits to a number of communes in 1983, this author was informed in each case that 'we built our water conservation system during the Great Leap' and that yields had stabilised and steadily improved with assured irrigation. See Patnaik 1988a. See also J.E. Nickum's 1974 thesis on 'A Collective Approach to Water Resources Development—the Chinese Commune System 1962–1972'.
10. For an earlier comparison of Indian and Chinese grain output per head see Patnaik 1988b.
11. China's gross per capita output (total output divided by total population) at 199 kg is seen to be a little lower than net rural per capita output at 204 kg (gross output minus procurement divided by rural population) because even in the worst year, 1960 the share of grain output which was procured, was less than the share of urban population in total population. Normally per head grain consumption is higher in rural than in urban areas and remained so in 1960 as well though the margin was reduced.

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Uses of History: India's Contribution to the Study of Climate and Disease

Elizabeth Whitcomb

The task of understanding the complex relations between climate and disease is now recognised worldwide as a matter of urgency. India is uniquely placed to make a seminal contribution. The meteorology of that most fascinating of weather systems, the monsoon, has been systematically observed with unrivalled range and precision over the south Asian subcontinent from the establishment of the India Meteorological Service in 1875. These exceptional data are matched, in quantity and quality, by observations of central and provincial medical officers of the departments of the sanitary commissioners (later public health) with vital statistics consistently recorded monthly from the establishment of registration in 1864. Annual meteorological and medical reports contain, in addition, detailed descriptive detail in text which accompanies, and fills out, the statistical tables of monthly observations. The unique British Indian archive is richest perhaps in material from which we may reconstruct the meteorological and medical history of the greatest killer diseases of the subcontinent: cholera and malaria. Our reconstruction can draw on a comprehensive series of monographs on climatic—oceanographic and atmospheric, biological—marine and terrestrial, to establish patterns of events in the modern history of vector-borne diseases, the precise role of climate and the limits of climatic determinism.

The mound of available data is immense. A chance finding, that in certain famine years there is a highly significant association, in the extreme conditions of these years, between climatic events and in the pattern of principal causes of disease, sets a hypothesis with which we can begin to explore it.

The subcontinent of India in the late nineteenth century was visited repeatedly by severe and widespread famine, with death rates reaching extraordinary levels of 25 even 35 per mille. On closer inspection of the official records for years marked by the most catastrophic of famines, records distinguished by meticulous medical and meteorological observation, a peculiar pattern of climatic conditions emerges not merely drought, prolonged and persistent, but drought interrupted, at irregu-

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